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METHODOLOGICAL PROBLEMS IN RESEARCH ON BEHAVIOR CHANGE

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Research on behavior change presents some of the most complex and difficult methodological problems in the behavioral sciences. Attempts to solve these problems inevitably raise basic issues of scientific inquiry -- naturalistic observation vs. experimental research, process observation vs. outcome observation, idiographic vs. nonothetic definition of variables. In this chapter we will explore the implications of these issues for behavioral change research. In doing so we will rely primarily on outcome research in psychotherapy for our empirical examples. While psychotherapy research is not the only area which uses the outcome research paradigm, it is by far the most contraversial. For example, Eysenck says, "With the single exception of the psychotherapeautic methods based on learning theory, results of published research with military and civilian neurotics, and with both adults and children, suggest that the therapeautic effects of psychotherapy are small or non-existent, and do not in any demonstrable way add to the non-specific effects of routine medical treatment or to such events as occur in the patients' every day experience." (1961, p. 720)

This review of methodological errors and problems will seriously question any such overall conclusions about the effects of psychotherapy, either positive or negative. This is not to say that there are no good studies on the effects of psychotherapy. The problems arise when one makes conclusions about the general effects of all forms of therapy on patients in general. Before statements of this sort can be made it is necessary to solve problems of control phenomena and control groups and to consider outcome criteria. Of special interest are possible Type II errors (saying therapy had no effect when in fact it did) in therapeautic

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outcome research. Many factors besides the therapy itself may be responsible for failure to reject the null hypothesis -- using criterion variables not relevant to therapy, improper comparison groups, and tests which are loaded against change are a few.

PROBLEMS OF EXPERIMENTAL DESIGN

Design Structure. Meehl (1955) gives the following as requirements for outcome studies in therapy, "The minimum standard for an adequate outcome study obviously includes: a) a control group; b) pre and post therapy evaluation procedures which are either objective or if judgemental, are uncontaminated; c) followup of both groups, preferably repeated so that exacerbation and remission rates can be estimated and the curves extrapolated." We will have occasion, in the next sections, to examine these criteria and to access their applicability and validity.

The design structure Meehl proposes looks something like this:

$$\begin{array}{ccccc} O_1 & T & O_3 & O_5 & (O_7) \\ O_2 & & O_4 & O_6 & (O_8) \end{array}$$

where O equals some observational technique and T equals some form of treatment. Ignoring for the moment the follow-up ($O_{5,6,7,8}$), let's examine the basic pre-post control design. This design has stood for many years as the model for outcome research in nearly every area -- from opinion polling to therapy research. It serves well, providing potentially adequate control for many factors which could explain changes in the experimental condition independent of the treatment. Campbell (1957) lists some of the more important of these variables --

1. History. The history of the experimental group members may cause treatment differences, e.g., differing motivation for therapy.

2. Maturation. Subjects change systematically from pre-test to post-test, e.g., the child in therapy grows out of his problems.
3. Testing itself. Here Campbell makes the distinction between "reactive" and "non-reactive" tests. Reactive tests are those which influence the subject (e.g., the subject is interviewed about his psychological complaints.). Non-reactive are those which the subject is essentially unaware of (e.g., ratings of the subject made by his boss). Without controls one cannot say whether a reactive pre-test or the treatment causes the change in the treatment group. A most obvious example would be IQ tests given before and after therapy. What produces the change in IQ?
4. Instrument decay. The most common examples of this variable lie in ratings. From pre to post testing judges can become more experienced or more fatigued, thus introducing systematic differences in pre and post ratings.
5. Statistical regression. Many times groups are selected for a particular treatment on the basis of extreme scores on O_1. In this manner, extreme high scorers on the MMPI neurotic triad might be selected for psychotherapy. The natural trend for extreme scores is to regress toward the mean on a retest.

It should be pointed out that to adequately control for these variables all subjects must come from the same population and be randomly assigned to the experimental and control groups. This, however, is an ideal which is seldom realized. In the section on control groups we will see some of the reasons why. One of the values of spelling out the specific variables to be controlled is that alternative designs and controls can be used to cover all irrelevant variables when one design cannot be perfect. For example, Rogers and Dymond (1954) in their studies on personality change after client-centered therapy used the patient as his own control to control such variables as motivation for change and unique personal histories, and used another control group to control for passage of time and maturation.

But even if the logic of the experimental design proposed by Meehl could be perfectly fulfilled; in some cases, it would still be inadequate. When the pre-test is reactive (and nearly all tests are to some degree) we are faced with an interaction effect between the test and the treatment.

For example, if the patient is given a self-ideal self Q-sort before therapy, in the act of taking the test he may become aware of the discrepancy between what he is and what he wants to be. This awareness may increase his motivation to be helped and he may thus profit more from therapy than someone who has not had the pre-test Q-sort. Campbell (1957) cites experiments in which this type of interaction has been demonstrated.

Solomon (1949) has developed a modification of design which permits the reactive pre-test effect to be measured and partialed out. This is the four-group design. Its structure is described by Campbell (1957) as follows:

O_1	T	O_2
O_3		O_4
	T	O_5
		O_6

Here all four groups are assumed to come from the same population and the two treatment conditions are assumed to be the same. A simple analysis of variance allows the test for treatment effects and for pre-test effects:

	No T	T
Pre-test	O_4	O_2
No pre-test	O_6	O_5

To test the effects of maturation and histroy use t-tests comparing O_6 with O_1 and O_3. This design thus becomes a new standard for the outcome research. But its complexity makes one wonder at its practical use in research, especially when Meehl says of his comparatively lenient criteria: "I am saddened to report that perusal of over two hundred journal articles and a dozen books reveals one paper approximating these desiderata (relaxing

requirements by forgetting follow-up but insisting on controls does not change this figure.)"

Campbell (1957) mentions another design which can control all of the above mentioned variables and also avoid the pre-test problem. This design involves random assignment of subjects to experimental and control groups at some point before treatment begins (A) and elimination of all pre-testing: vis. --

A	T	O_1
A		O_2

If assignment to groups is truly random, then any differences between O_1 and O_2 are attributable to T. The major problem with this design is that it is less precise and less sensitive to change; but it does offer a real alternative to pre-test designs.

Turning to questions of follow-up in outcome research, Meehl's requirements give an ideal though perhaps unattainable set of criteria: "follow-up of both groups, preferably repeated so that exacerbation and remission rates can be estimated and the curves extrapolated." The practical problems involved in follow-ups appear almost insurmountable. Sargent (1960) discusses patient attitudes which hamper accurate reporting in the follow-up interview. Most patients want to forget the time when they were sick and are likely to see continuing follow-up as an invasion of their privacy. Attempts to interview friends and associates of the patient are likely to be even more offensive. In addition, there is likely to be a good deal of 'selective forgetting' in reporting information.

On another level there is the more basic problem of finding the person after he leave treatment. Results at the Phillips Psychiatric Clinic

at Johns Hopkins (Frank, 1958) are typical. One year after therapy they were able to contact 53 out of 54 patients; at two years, 48 out of 54. "Since then attrition has been marked." This comes close to Meehl's ideal, but it is still doubtful whether meaningful exacerbation and remission curves could be extrapolated. As one might expect, there are systematic biases in patients who are not contacted in the second year. "The importance of the patient's attitudes may be illustrated by the fact that after two years we were able to obtain re-interviews with 90% of the patients who had originally accepted therapy, but only 33% of those who had dropped treatment." (Frank, 1960)

If we consider the reactive pre-test issue, other complications arise. Since the post-test will act as a pre-test for the follow-up, a continuation of the logic of Solomon's design would require the addition of four groups for each follow-up period; in the random assignment/no pre-test design, two additional groups are required for each follow-up. Add to these difficulties the fact that few researchers are particularly interested in waiting for two to four years to complete their project -- and the follow-up problem seems overwhelming.

An interesting follow-up strategy was used by Leay and his co-workers (Leary, et.al,1962) to make a significant attack on these problems. His game-model theory of behavior change includes all testing, including follow-up, in the therapy procedure. In this way the reactive pre-test problem is neatly avoided, since all people who come into this type of therapy receive pre-testing. Some of the problems of defensiveness in the follow-up are alleviated since follow-up is part of the original therapy contract and is seen as a continuation of therapy. While this approach

has its own unique problems (e.g., bias resulting from the patient's telling the therapist what the therapist wants to hear), it offers an alternative to conventional follow-up procedures.

Controls. The concept of "controls" seems almost synonomous with "control groups". But as we suggested earlier, it might be more useful to work with the variables to be controlled -- what Rogers and Dymond call "control phenomena". In their words:

> "....the important concept regarding controls is not so much the concept of control groups as the concept of control phenomena -- that is, the importance of controls lies in adequate accounting for variables presumed to be relevant to therapy. Such controls can be instituted in many ways. Sometimes it means using separate control groups. At other times the best controls may come from using different measures of the same person, that is, the 'own control' method. In still other instances controls lie in data not in people. For example, if one wishes to describe those points in therapy which counselors characterize as deep, an essential part of such a description comes out of a comparison of such points with randomly selected passages. Finally, effective control methods lie in statistical controls as a supplement to experimental controls. For example, if one wishes to institute controls when continuous measurement is involved (e.g., recognition time in a perception experiment), a method of choice may be analysis of covariance where the effects of initial differences between experimental and control groups are partialed out and the groups equated by statistical means." (1954, pp. 21-22)

In addition to the above control techniques it is useful to further divide Rogers and Dymond's "separate control" groups into three types:

1. Random assignment control groups. Subjects from the same general population are assigned randomly to experimental and control groups.
2. Stratified sampling controls. In this design, relevant variables are controlled by having them occur in the same proportion in both the experimental and control groups.
3. Matched controls. In this design each experimental subject is matched with a control subject on relevant variables.

The last two designs present an interesting dilemma. While controlling relevant variables increases precision by making the two groups more similar on the matched variables, the matching procedure violates the principle

of random assignment and increases the probability that the two groups may be significantly different on other variables (Selltiz et.al., 1959).* Frank (1960) questions whether it is worth the while to match controls because research in psychotherapy is in such a preliminary stage that we do not know what the relevant variables are. He cites the previously unrecognized effect of social class on the therapeutic process as an example. Brunswik (1956) also makes an important point in this connection. Most psychological variables, he says, are tied to other variables (e.g., social class with delay of gratification, IQ, etc.). The manipulation of one variable manipulates other known and unknown variables in unknown and perhaps unnatural ways. For this reason the matching of subjects not only makes generalization difficult, but also may manipulate variables the researcher is not aware of, which will in turn cause differences which will be attributed to the known variable manipulated in the matching process. These considerations do not invalidate matching methods, but do suggest great caution in using them.

In addition to control problems in selecting variables on which to base the choice of the experimental and control groups. There is a similar problem in specifying the variables involved in the treatment condition. Failure to specify carefully can lead to erroneous generalizations. Eysenck (1961) lists four types of variables which may be peculiar to the experimental group. None of these is covered in most theroetical discussions of what happens in therapy.

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*This of course need not be so if the experimental and control subjects are selected randomly from a large sample of people who are matched on the variable -- a highly impractical procedure.

1. physical examination and the medical treatment of all sorts of minor illnesses, etc.;
2. long periods of rest in hospital, clinic or other institution;
3. manipulation of the environment on the part of the doctor, including attempts to change the attitudes of family, employers, etc. (Klumpner, 1955);
4. more regular and better-balanced food in hospital compared with previous existence (cf. the paper by Watson and Camney, 1954, on the effects of nutritional replacement on neurotic disorders." (p. 702)

Though these variables do not confound all studies, Eysenck's point is well taken. Over emphasis on unvalidated theories about the process of change may cause us to overlook other important aspects of the treatment process.

Another phenomenon which may work to produce differences in the treatment condition regardless of the treatment is the therapy drop-out. With all control methods but the own-control design the person who drops out of therapy constitutes a major biasing factor toward a Type I error. These people are self-selected and in one sense represent therapeutic failures (assuming they don't get better). If you don't include these failures in the experimental group totals -- and you can't because they drop out before the post-test -- then you exclude potentially low changer, thus artificially increasing the average experimental-group change. What controls do you throw out to counter this bias? At this stage of the game it's not really clear. One of the values of research attempts to identify abrupt terminators (e.g. Barron 1953, Afflect and Mednick 1959) is that they can be used eventually to eliminate those control subjects who would be abrupt terminators in therapy, thus eliminating the bias.

Let's concentrate now on a few common control-group procedures, assessing their effectiveness as experimental strategies. Perhaps the most common control group used in studies of psychotherapy is the waiting-list control. This technique calls for the random assignment of patients on

the waiting list to therapy and control groups. There are, however, four discernable biasing effects which work against their random assignment.

1. For various reasons the interviewers who assign patients to treatment and control groups often will pick for therapy those who appear to be the sickest and in the greatest need. This would appear to create a clear bias toward a Type II error. We cannot, however, be certain of the direction of the bias, in the light of Luborsky's (1962) findings that anxiety and discomfort are positively related to improvement in therapy.
2. A Type I error bias is also introduced by the interviewer who assigns patients. A growing body of research suggests that therapists tend to prefer patients who have characteristics which will help them improve -- e.g., higher IQ, higher ego-strength, psychological-mindedness, higher social class, etc. Thus there may be a selection factor in treatment assignments which gives therapy to those patients who will get well anyway. (See Sharaf and Levinson 1957, Schaffer and Myers 1954, Levinson 1962).
3. A third biasing factor results from the fact that the really disturbed and uncomfortable people on the waiting list don't wait. They seek help elsewhere. Sometimes they stay in the research project; sometimes they leave altogether. But either case biases toward a Type II error; in the first instance controls are receiving therapy, in the second the most disturbed members of the control group are not included in the sample.
4. Finally, there is some question whether the waiting list is really a no-treatment control. It would seem that interviewing, testing, etc. may in fact have therapeutic value (Goldstein, 1960). This possibility will be discussed more fully in the section on selected effects.

The own-control method (e.g. Rogers and Dymond 1954) nicely avoids the aforementioned problems of the waiting-list control and some others, e.g. differential motivation for therapy. Unfortunately, however, it has problems of its own. First among these is a problem similar to that caused by the reactive pre-test. It is almost certain that some kind of interaction must take place between the waiting period and the therapy. Dissonance theory would predict that painful waiting for therapy would increase the value of the therapy and perhaps the expectation that it will help (Festinger and Aronson 1960). The patient becomes the aspiring knight who

must do penence before entering the sacred shrine (cf. Frank 1959). If there is anything at all to these speculations, then we are faced with a problem of generalization. If the patients improve more in therapy than in waiting, are we justified in saying that the therapy alone is more effective than no treatment?

A second problem with the own-control method is more obvious. It provides no control for events that occur during the course of therapy. For example Frank (1958) suspects that neuroses may get better during the summer months (he uses drop-off in intake percentages during summer months as data). Similarly, it appears that neuroses go into remission after about a year (Eysenck 1961). Thus a patient may "hit the bottom" near the end of the waiting period or at the beginning of therapy and gradually improve after that. In the same way, young patients may grow out of their difficulties.

Finally, to control for passage of time, the waiting period should be as long as the therapy. There are two tricky issues here. It is unrealistic to keep patients waiting for therapy for extended periods of time. And how do we know in advance how long therapy will be?

A third type of control which deserves discussion is the use of population base rates. Proper use of this control permits generalizations of the following type: Treatment X shows more (or less) improvement than base rate of improvement in tha absence of that treatment. While from a practical point of view this method provides a very effective practical score-keeping device, it is not as useful from a purely scientific basis. The basic problem lies in the specification of the nature and causes of the base-rate improvement (spontaneous remission). In other words, in

most cases one cannot specify the nature of the treatment given base-rate controls. Denker's attempt to give base-rates for recovery from neurotic disorder will serve as an example of the difficulties in this specification (reported in Eysenck, 1961). Denker studied 500 consecutive disability claims due to psychoneurosis, taken from the files of the Equitable Life Assurance Society of the United States. These patients were treated only by general practitioners, not psychotherapists. He found that 45% of the patients recovered after one year, another 26% after two years, another 10% after three, another 5% after four, and another 4% after five years. His criteria for improvement were: "a) return to work, and ability to carry on well in economic adjustments for at least a five-year period; b) complaint of no further or only very slight difficulties; c) making of successful social adjustments." (Eysenck 1961, p. 710). Even if we accept these criteria as reported -- and there is perhaps question about bias in reporting to one's insurance company -- there is certainly question as to what caused the return to health. Eysenck seems content to call it spontaneous remission and leave it at that. A critique of Denker's study by Cartwright (1956), however, makes the remission seem anything but spontaneous. Cartwright points out that Denker's neurotic patients were selected at the peak of the Depression, the follow-up carrying over to the early 1940's. Since one of Denker's major criteria was employment, it would seem reasonable to attribute remission of these patients to increasing employment opportunities after the Depression. In addition, it is unreasonable to think that these patients went without 'therapy'. Even the most 'general' practitioner is willing to hear a patient's troubles. Similarly, there are always friends, relatives, ministers, etc. They too can help when the need arises.

Selected effects -- Spontaneous remission; expectations; the placebo effect. Let us turn now to a set of related variables which have tended to produce Type II errors in outcome research. We can best begin with an example. Barron and Leary (1955) did a pre-post outcome study with 150 psychoneurotic patients who had applied for treatment. Eighty-five received group therapy, 42 received individual therapy and 23 patients were placed on waiting lists and served as controls. Criteria for improvement in this study was change in MMPI profile. At the post-test (after about eight months) the therapy patients showed significant decreases on the depression, hysteria, hypomania and lie scales of the MMPI (both individual and group therapy patients). In addition, group therapy patients decreased significantly on paranoia and psychasthenia. Both therapy groups showed significant rises on the ego-strength scale. The control patients also showed improvement. Depression, psychesthenia, and hysteria scales decreased slightly more for the controls than for the therapy groups. Ego-strength scores of the controls increased significantly. Barron and Leary conclude "for the most part .... the changes tend to be in the same direction for treatment and nontreatment groups, and of about equal magnitude."

We have already discussed many methodological problems which would make the results of this study questionable. It is interesting, nonetheless, because of the marked improvement which the controls show, and because of Barron and Leary's speculation as to why this improvement occurs. They say: "In a sense, of course, simply having committed oneself to participating in psychotherapy, and having a reciprocal commitment from a clinic to psychotherapy, even though not immediately, represents a

breaking of the neurotic circle. A force for change has already been introduced. In addition the initial interview and the psychological testing may themselves be therapeutic events." This paper represents on of the first recognitions of the so-called placebo effect in psychotherapy research (cf. Shapiro 1960, Rosenthal and Frank 1956, Campbell 1957).

The placebo effect is of concern to outcome research strategy because it seriously challenges the possibility of a 'no-treatment' control. The previous discussion of controls was based on the assumption that the control group receive no treatment. In discussing the various control groups, however, we saw that untreated patients will probably be involved in some kind of therapy-type interactions with friends, relatives, etc. Now it appears that the very act of setting up a control group could be a kind of therapy. As Frank (1959) points out, a good deal of the change in the patient may be a result of the patient's expectation of change and his perception of the potency of the therapeutic institution. Goldstein (1960) has listed several control-group activities which could be pseudotherapeutic. Controls have been given any or all of the following:

1. Intake interview
2. initial psychological testing
3. social-work interview
4. periodic and/or post wait period psychological testing
5. post wait psychiatric interview
6. interviews with patient's relatives or friends.

Goldstein gives a formula for control-group improvement similar to Frank's. Improvement is a function of the patient's degree of favorable expectation of improvement from psychotherapy and the extent to which he receives professional attention via nonspecific placebo-like activities.

To overcome this problem a shift in research strategy is required. We must begin thinking in terms of comparing one type of therapy with another rather than comparing therapy with 'nothing', (Frank 1958, Campbell 1957, Goldstein 1960). The recognition that therapy is only one of many types of diadic interactions represents a major research breakthrough. It opens the door to the study of within-therapy variables such as patient and therapist expectations, length of therapy, and theoretical orientation; and their effects on outcome.

A second source of bias lies in differential evaluation of experimental and control groups resulting from the therapy given to the experimental group. A study by Walker and Kelly (1960) will serve as an example. It is reviewed by Strupp (1963) as follows: "a group of newly admitted male schizophrenic patients in a Veterans' Administration hospital (N = 44) who received short-term psychotherapy within six months after admission were compared with a control group (N = 38) who did not. Whereas the groups were considered roughly equivalent, actually they diverged sharply in terms of prior hospitalization (median of 2.8 months for the therapy group, 15.5 months for the control group). Criteria were ratings of symptom improvement, ward behavior, discharge from the hospital and posthospital adjustment after 90 days. No differences in improvement were found, except that significantly more control group patients were discharged within six months of admission. The authors report that apparently therapeutic goals were set higher for therapy patients than the controls, which may account for their staying longer in the hospital. At follow-up no differences were found between the two groups."

Here is an instance in which we see one of the most objective criteria for improvement, discharge from the hospital, was biased against therapy patients. It appears possible that therapists, in their desire to help the patient, keep him in the hospital longer, and in this sense make him do worse than untreated patients. One cannot help but wonder if this phenomenon does not occur in other criteria for improvement. We may very well have here a particular instance of what Winston White calls relative deprivation. This is the phenomenon in which people who have had very little of a commodity (e.g. subjective feelings of self-satisfaction and comfort) raise their level of aspiration very high when they are given a small quantity of the commodity. White writes of how this phenomenon leads to dissatisfaction and unrest in underdeveloped countries. The same may be true of the perspective therapy patient. Having made a therapy contract, the patient may feel a certain amount of relief. Now he can begin to want new comfort, to be more perfect, or normal. What effect does this have on his self-evaluations (Q-sort, MMPI, etc.)? He will most likely become more conscious of his faults and more painfully honest about himself; his self/ideal self correlation will decrease; in short, he will tend to see himself negatively. Thus it may be that even though controls and experimentals do not differ on objective criteria, the experimentals may be more 'honest' and use more stringent subjective criteria (e.g. - Barron and Leary found significant decreases in the MMPI lie scores for the therapy groups, while not for the control group).

One way out of this problem is to use evaluation scales in which the patient (relative, therapist, ward personnel, etc.) defines his evaluation criteria for the researcher. In this way changes in evaluation criteria

and anchoring points can be noted. The self-anchoring scale developed by Kilpatrick and Cantril (1960) and some modification of Kelly's REP Test (1955) are potentially useful here (see below).

EVALUATION OF OUTCOME -- THE CRITERION PROBLEM

The development of suitable outcome criteria is perhaps the most pressing problem in behavior change research. It is not the purpose of this section to review the various kinds of outcome measures, since this has been done quite adequately in a number of other articles (Scott 1958, Scott 1962, Cox and Klein 1960, Cohler 1962, Eysenck 1961, Forsyth and Fairweather 1961). Instead, the emphasis is laid, once again, on methodological problems in defining the criterion and the resulting strategy of research. The selection of criterion variables inevitably involves consideration of some basic strategic alternatives in the process of scientific inquiry. We will consider three of these.

Naturalistic vs. Hypothesis-testing Research

The issue here is whether it is best to observe as many changes as possible and then try to interpret them, or whether one should formulate specific hypotheses and interpretations in advance and test them by gathering relevant data. The advantages of the first strategy is that it is very sensitive to change -- it can pick up changes which existing theories do not predict. The disadvantage is that it becomes difficult to specify what variables produced the change. The second stragegy improves the ability to isolate variables but increases the probability that some changes will not be detected, because the theory does not predict them. Thus the statement that psychotherapy does not produce any change in the individual is not justified. It could just as well be theories we have

about what happens in psychotherapy are not able to predict the changes that do occur (cf. Edwards and Cronbach 1962, Campbell 1957).

Most researchers, however, agree that this is not an issue of either/or but a matter of sequence. They point out as do Butler et al (1963) that the development of all science is from naturalistic observation to hypothesis testing. The question then becomes, "When have we observed enough to begin testing hypotheses and formulating variables?" It is only in the face of rather dismal and often times negative results (e.g. Eysenck 1961) that we are beginning to shake off oversimplified preconceptions and face the tremendous complexity of the therapy dyad (cf. Butler et al 1962, Leary and Gill 1959, Robbins and Wallerstein 1959, Luborsky 1959).

Nomothetic vs. Ideographic Approaches to Evaluation

This decision refers to strategy in selecting dimensions on which to measure change. Should one use variables derived from general personality theories -- variables which we assume to operate in all individuals in general, lawful ways? Or should we consider variables which seem relevant to the individual patient, whether or not they are potentially useful as general psychological laws? The nomothetic approach has the advantage of giving comparable scores, while the idiographic approach presumably gives only qualitative differences. The danger of the nomothetic approach is its potential insensitivity to change. As Krech et al (1962) point out, it is very difficult for someone to change on a dimension which is not relevant to him. In addition it is difficult for a person to tell how he has changed if he isn't asked the right questions.

Since idiographic methodology has only recently gained acceptance by psychological researchers, it may be useful to describe the idiographic approach to personality assessment in some detail and to describe some specific idiographic research methods. Much confusion surrounds the idiographic-nomothetic contraversy. The source of this confusion lies in the fact that other similar but conceptually distinct issues break along the same lines as the idiographic-nomothetic issue. As a result idiographic and nomothetic tend to be defined in terms of these associated issues. Thus Holt and Meehl associate idiographic with subjectivism and intuition--nomothetic with objectivism and positivism. For Eysenck it is the literary vs. the scientific; for Maslow, May, and the existentialists it is reality vs. truth. For Allport it is the unique vs. the general. James speaks of "tender-minded" and "tough-minded" and Lewin of Galilean and Aristotelian modes of thought. Angyal distinguishes between systems analysis and relationship analysis; Brunswick between naturalistic method and experimental method. The list could most likely extend indefinitely.

While there is undoubtedly some "truth" in all of these definitions, the ambiguity that arises from them renders the idiographic-nomothetic distinction relatively useless for scientific purposes. Though it is perhaps arbitrary let us for purposes of this discussion, define the idiographic approach in terms of two principles.

1. The idiographic approach is concerned with studying the patterning of components within a single individual, as opposed to studying the correlates of a particular component or group of components across individuals.

2. The idiographic approach attempts to minimize theoretical and methodological distortion of the subject's naturalistic behavior.

The implications of these two principles can best be seen in the research strategy which results from them. Six factors are important here (after Katz 1962).

1. Stimulus Breadth. The range and variation of the stimuli presented to the person should be such that the person is encouraged to respond throughout the range of his response capacity in a way representative of his natural behavior patterns.

2. Situation Breadth. The individual's behavior should be assessed in varied and representative situations. The goal is to understand the whole of human behavior, not just human behavior in the psychological laboratory or the personality inventory.

3. Response freedom. This principle is intimately related to the first two. The individual should be allowed to respond in ways which best communicate his reaction to the situation. In many cases one cannot express his best opinion by answering true or false.

4. Sensitive recording and analysis of data. This, of course, is a major concern of all science -- but for the idiographer it is of primary importance. No matter how much care is taken to observe the first three principles; all is lost if the data collected is not representative of the situation, if "unnatural" statistical methods combine the data in artificial ways, or if the findings are "trimmed" to fit into the neat compartments of existing theory.

5. Collaborative research design and measurement. Rollo May quotes Straus, "the unconscious ideas of the patient are more often than not the conscious theories of the therapist." (1958, p. 5) In order that he not make the subjects a projective test of his theory, it is imperative that the idiographer attempt to gain the subject's point of view. Ideally the subject and researcher should collaboratively decide what is important to study and the best way to study it. A joint agreement of this type constitutes a long step in resolving the dilemma presented by the experimenter-subject roles. Kelly (1955) neatly illustrates this dilemma in psychological testing -- "In projective tests the experimenter guesses what is on the subject's mind; while in objective tests the subject guesses what is on the experimenter's mind."

6. Contextual emphasis. This last principle represents the idiographic conviction that behavior is best understood by understanding the situation in which it occurs. Understanding the demand characteristics of the situation is absolutely essential to the understanding of a subject's behavior. This principle is best illustrated in the work of Lewin, Angyal, and Cantril's transactional approach.

How can the idiographic approach as defined above be useful in the assessment of behavior change? Two factors seem especially important. First, the strategies outlined above should maximize the possibility of detecting significant changes in the patient's life. Statements about an individual are not just true or false--they differ on a dimension of saliency. Some areas are more important to a patient than others. By presenting a standardized list of problem areas and weighting answers in each of the areas equally we are increasing measurement error in the individual case. And we consequently reduce the possibility that the measure will detect changes in the patient. It is difficult to conceive of how a patient can show systematic changes in an area which is not salient for him.

Properly conducted idiographic research can circumvent this difficulty by a collaborative decision on the important problems which a patient has. By deciding with the patient the relative importance of these problems and the situations in which changes in them might be reflected we should maximize the possiblity of detecting significant changes.

The second factor has to do with the usefulness of idiographic strategy in discovering how the process of change takes place. As such idiographic assessment would constitute an intermediate phase preceding the nomothetic placement of the individual on some dimension of mental health (usually culturally defined). We suggest here a distinction between the assessment of change and the evaluation of change. The first process is essentially idiographic--attempting to discover what changes take place in a particular patient when his particular therapist uses a particular set of techniques. The second process is nomothetic -- Given the various

kinds of changes which take place in different patients, how do these changes affect the patients' effectiveness in terms of some mental health criteria. The use of the idiographic approach in the intermediate stage opens the door to the study of the processes involved in the therapy diad.

The following are a few examples of some recently developed idiographic methods selected because of their obvious relevance to behavior change research.

The self-anchoring scale. (Kilpatrick and Cantril 1962) The self-anchoring scale is perhaps the best known of the morphogenic methods. It is included here because it is an excellent example of the use of the response freedom principle without the loss of nomothetic comparison power. Whereas ordinary rating scales dictate for the subject the variable they are rating (e.g. self-respect) and assume the meaning of the variable to be the same for all subjects, Kilpatrick and Cantril ask the subject to give as complete a definition as they can of both ends of the continuum represented by the variable. Thus, in psychotherapy research we might ask the patient to describe the best person he can imagine and the worst person. Then by showing him a 10 point rating scale the ends of which are defined by his two descriptions, we can ask him to rate his present self, his self five years ago, his ideal self, etc. Note that we get two kinds of data here -- a description of the person's salient concerns and ideals which define the variable (these definitions can be content analyzed to get an empirical definition from a large population) and a rating on the variable which is comparable across individuals.

The personal questionaire. Shapiro (1961a, 1961b, 1961c) has developed a questionaire method to measure changes in psychiatric patients

which utilizes the joint decisions of psychologist and patient to construct a questionaire which is custom built to assess meaningful changes in the patient. Shapiro claims that his method can measure clinically relevant and highly individual symptoms, measure these symptoms so as to allow for comparison between changes in different aspects of the same patient, and can measure them so as to allow for comparison between changes in different subjects. There are five steps in constructing a personal questionaire.--

1. An interview with the patient in which the patient talks about his illness. This produces a list of the patient's symptoms in his own words.

2. An interview with the patient's psychiatrist to further clarify these symptoms.

3. Construction of a trial questionaire. Each symptom statement made by the patient is supplemented by two others -- an improvement statement and a recovery statement. An additional methodological refinement is the scaling of each of these three statements by the method of equal-appearing intervals on a continuum of unpleasant-pleasant.

4. Conducting a scaling experiment to make certain that the patient correctly rates the symptoms on the pleasantness dimension.

5. The final form of the questionaire is administered at various times in the patient's treatment career.

Kelly's REP Test. The best known of the factor analytic techniques for the single case is Kelly's non-parametric factor analysis of the Role Construct Repertory Test. A step by step procedure for this analysis has recently been prepared by Kinne (1963). In the Rep Test the subject is given a list of roles (e.g., mother, father, girl friend) three at a time. His task is to tell how two of them are alike and different from the third. Once a number of these constructs are elicited the subject is then asked to tell which roles are described by which constructs. The resulting role-

construct matrix is then subjected to a non-parametric factor analysis. The major factors can then be extracted, as well as the subjects perceptions of the various role members, his identification figures, and many other interesting variables. The chief advantage of this and other methods of single case factor analysis in the assessment of change is that they allow changes in the saliency and relationships of different variables to be demonstrated (i.e. changes in the factor structure), as well as changes in intensity of particular variables. The problem, of course, is that you get no more out of a factor analysis than you put in it. Allport's (1962) criticism of the Rep Test is that the subject is not given the response freedom to choose representative stimuli (roles to respond to. The Rep Test does, however, have an advantage over the two methods to be discussed next in that it does allow the subject to create his own constructs. In Block's approach the Q-sort items are standard and in Nunnally's study the Q-sort items are selected for their relevance to the person by the psychologist.

The factor analysis of Miss Sun. From interviews and tests Nunnally (1955) developed 60 Q-sort items especially relevant to the case of Miss Sun. He administered these to her according to standard Q-sort procedure at different times under different instructions before and after therapy. By the method of inverse factor analysis he was able to extract three independent factors which showed moderate change in therapy. They were:

I. A general mode of behavior factor -- aloofness, cooperativeness, and imperturbibility

II. An ideal-self factor -- outgoing, striving, and practical

III. How she thought she was seen by her friends -- a "Bohemian escape" from the conflict represented in factors I and II

Nunnally was also able to investigate the relationship between changes in the three factors. Contrary to expectation he found that Factors I and II changed together (Rogerian theory would predict that the present self would move toward the ideal self).

Block's approach to single case factor analysis. (1952, 1953, 1961) Block and his subject (a medical secretary) choose the nine people with whom she spent the majority of her time (the method called for by Allport). He then asked her to do a Q-sort, using 100 pre-selected items, on how she felt and behaved when she was with each of these nine people. From these sorts he produced a 9 x 9 correlation matrix which he subjected to factor analysis. Since current factor theory suggests that the first unrotated factor can be considered a "g" factor, Block took this factor as "the core set of behaviors and feelings underlying all of the S's interactions .. (to serve) as the frame of reference against which various group factors may be contrasted in order to discern the items characterizing each factor":(Block 1952, p. 277). In this manner he was able to discern how much his subject "played a role" with specific others and what that role was.

In a second study Block (1961) used the amount of variance accounted for by the "g" factor as a measure of Role Variability (RV) (high RV equals low "g"). He predicted a curvilinear relationship between RV and psychoneuroticism as measured by a scale made up of MMPI and CPI items -- both high and low RV people should be neurotic. He found instead a negative linear correlation between "g" and Psychoneuroticism showing that high RV people tend to be more maladjusted (r-.52). He related his findings to Erickson's concept fo ego identity. Block's approach in this study

is a good example of how idiographic changes in therapy (e.g. RV) could be related to nomothetic evaluative variables.

Objective vs. Subjective Change

This last decision to be made in selecting outcome criteria turns on the assumed validity of subjective report. One camp says that they are interested in behavior change, not changes in subjective report. The other camp says they are interested in personality change, not symptom relief. Contrast Rogers' long commitment to the validity of the patients' subjective statements to Eysenck (1961) who says, "It is obvious that the more objective the method, the more trustworthy will the results be. Thus a questionnaire would be preferable to an essay." Most attempts to find relationships between objective behavioral indices and subjective report fail to find consistent results (Forsyth and Fairweather 1961, Kelman and Parloff 1957, Parloff, Kelman and Frank 1954, Scott, 1958a). Outcome research which uses both objective and subjective criteria often shows change on one and no change on the other. A particularly good example of this is an experiment done by Heller and Goldstein (1961). To measure changes in dependency during therapy they gave patients a subjective measure of dependency (the Edwards Personal Preference Schedule Scales for succorance, deference and autonomy) and a behavioral measure of dependency (a role-playing form of the Rozenweig P-F test). The post therapy results indicated that patients had changed in subjective report of dependence. On the behavioral measure of dependence, however, there was no change. Rogers and Dymond (1954) find similar results.

> "It was found that there was no significant difference between the pretherapy and post-therapy behavior of our clients, on the average,

> according to their friends' observations....When the clients' ratings of their own behavior were compared with the ratings by their friends, it was discovered that our clients consistently rated themselves less favorably than did their friends: but this discrepancy steadily diminished so that by the follow-up point their perception agreed much more closely with that of their friends."

How do we explain this discrepancy between objective behavior and subjective report? We will look first at factors which may bias a subjective report and finally at possible biases in objective behavioral measurements.

Biases in Subjective Report. We have already discussed two factors which can influence subjective report -- relative deprivation, and constraints placed on the subject by the experimenter, so that he might obtain nomothetic data. An additional group of factors can be subsumed under the heading of reciprocal role expectations. The classic formulation here is Hathaway's hello-goodbye effect (1948). His general notion is that the patient, in his statements and evaluations, tries to legitimize his relationship with the therapist -- by acting out his perception of the therapist-patient role relationship. Specifically, the patient applying for therapy presents problems right away in order to justify the contact. He is likely to make the problems seem fairly serious, though there is no necessary relationship between the problems he states and his real reason for entering therapy. Later, as therapy draws to a close the patient will begin to summarize, wrap up old topics, hesitate to bring up new ones. The patient will imply that topics of conversation have been adequately treated; perhaps saying he feels better. In short, the patient will to some degree say what he thinks the therapist wants to hear.

Frank (1958) reports a bit of data from Rogers and Dymond which supports the general notion: "...the own-control clients who were placed in a sixty-day waiting period of no therapy were divided into two groups: the attrition group who stayed for less than six sessions of therapy subsequently and those who stayed for more than six sessions. On all measures the attrition group showed more imporvement over the wait period than those who later accepted therapy. This is interpreted to mean that the attrition group showed more tendency to spontaneous recovery. Another possibility exists, which is that at the second testing the remainders wanted to show that they still felt the need for treatment; the attrition group, that they did not want further treatment."

The relationship between subjective reports and behavior becomes further complicated if cognitive dissonance is considered. What is the effect of the patient hearing himself say he feels better? Frank (1958) says "If in response to factors in the test situation he says he feels better or worse than he 'actually' does, his feelings may change to conform with his behavior, as William James observed long ago in a somewhat different context."

The solution to these problems of subjective report lies, we think, in theories which interpret subjective report as a function of the situation in which the report is made. Kelman (1961) has developed such a theory -- one which has great potential utility in outcome research. He distinguishes three levels of attitude change -- compliance, identification and internalization. In compliance, the attitude change is demonstrated only under surveillance of the influencer (e.g. the therapist), in identification the change is demonstrated when the situation is of salience to

the influencing agent (i.e., when the therapist is likely to find out, as when a researcher tests the patient), in internalization the change is demonstrated in situations where it is relevant to issues (i.e., the patient demonstrates change in his everyday life). It is this last level of change which therapy aims for. But for the most part we have only tested change at the first two levels. To test the last level we need testers whom the patient does not perceive as related to the therapy.

Biases in objective measurement. In deciding the subjective-objective issue one often overlooks the fact that so-called objective measurement can also err. We will discuss some possible measurement errors in two types of objective criteria -- empirically derived tests such as the MMPI and CPI, and ratings. Researchers have long been aware of measurement errors in rating due to the rater's idiosyncratic definitions of the criterion variables, the rating scale, etc. Rather than rediscuss these issues let us focus on a more subtle issue, rater bias. A study by Brill et. al. (1957) on factors associated with improvement in electro-shock therapy will serve as an illustration. This study attempted to determine what factors were associated with improvement by systematically eliminating important variables in successive groups (in one group they did everything but administer the shock; in another, everything but strapping the patient down and administering the shock; in some they eliminated the muscle-relaxing shot, etc.). One of their criteria was ratings by ward personnel. Results in general indicated no significant differences in imp vement between groups, except in extreme cases. One of the difficulties in making generalizations in this study is the fact that it is difficult to tell whether ward personnel changed their ratings because of actual differences

in patients or because of their expectancy that patients would improve after electro-shock treatment. (This expectation should have been constant for all groups). The addition of another control group might test this hypothesis. Asking nurses who had just come to work to rate untreated patients who they were told had received electro-shock on the previous shift might give an estimate of the effects of expectancy. Another method would be to obtain independent estimates of the nurses' belief in the effectiveness of electro-shock treatment and correlate these with their improvement ratings. Expectancy effect could then be removed by partial correlation.

Another issue is the use, in change research, of personality tests constructed by the method of contrasting groups. It is a real issue, whether change scores on tests like the MMPI and CPI are meaningful. While the scales themselves are highly validated, there is little evidence to indicate that change on these scales represents any meaningful change in the person's personality. In fact, it has been customary to attribute test-retest changes to unreliability. In order for changes on these scales to be meaningful it must be demonstrated that these changes are associated in some predictable way with other changes in the individual way with other changes in the individual. This has not been done. And if the content of these scales is at all meaningful, it may be difficult to find such relationships. On some of the items it is difficult to conceive how change could be possible, or just what a change would mean: e.g., "I like tall women," "I like poetry," "I am a very ticklish person."

SUMMARY

The reader must by now have a certain sense of frustration for this chapter has raised many problems and offered few solutions. Yet there is little cause for despair and some cause for optimism. While the multitude of potential methodological errors makes it nearly impossible to design a single perfect experiment on behavior change, it is possible to cancel out errors by using a variety of methods to research the same problem. The Rogers and Dymond research (1954) that we have cited throughout this paper represents an excellent early example of this multi-method approach to research on behavior change. If our knowledge of behavior change technologies is to grow research must continue from a variety of methodological and theoretical points of view. Psychotherapy should strive for behavioral indexes of its success and learning theory therapies should assess the impact of their methods on self-report measures. Furthermore, we need more studies which relate the process of the behavior change technique, whatever its theoretical basis, to both behavioral and self-report outcomes. From this research should come processes which are common to all methods of behavior change.

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